

of primary sources in Persian provides us with evidence that under the Safavids the central bureaucracy had enfeoffed Darabjird, a major city sitting astride the route from Bandar Abbas to Shiraz, with the Purnak Turkmens of the Aqqyunlu confederation. Archival and narrative sources from the 17th century testify to the fact that by the 1620s all major urban centers in the Persian Gulf littoral, including Shushtar, Kazarun, and Laristan, had been incorporated fully into the *khāṣṣa* (crown) sector of the Safavid state. The author also disregards the dealings of the Safavid central authorities with the long line of Afshar and Georgian military chiefs who ruled in these cities over the course of the 17th and 18th centuries. Moreover, we know that under the Safavids the sedentary and nomadic inhabitants of the Persian Gulf littoral were subject to taxation and conscription. And anyone having read the Safavid narrative sources knows that conscripts from the Persian Gulf littoral were widely praised for their marksmanship and that in the 17th century several army units composed exclusively of these conscripts had been posted to Qarabagh and Armenia. One can discuss the ebbs and flows of administrative centralism in the Persian Gulf littoral under the Safavids, but to fancy that the whole region was “an alien territory” for the Safavids is simply fallacious. In a sense, such close reading of primary sources might not be necessary for someone who writes for pleasure. But a serious historian who makes big claims about the very nature of governance, economy, and culture in early modern Iran ought to be able to read these materials and “discern” their meaning and significance just as he does with the more biased European sources. I suppose a careful reading of such sources is too much to ask from a historian who has constantly trivialized Persian primary materials.

As I clarified at the end of my book review, the parts of the book that deal with Iran’s foreign trade and monetary system in the 17th and 18th centuries have the merit of being essential additions to the existing literature on Safavid history given the paucity of research in those subfields. I stand by my assessment, however, that any serious reader of the book will find a good deal of its arguments and conclusions without firm foundation in the Persian primary sources.

A COMMENT ON ALI YAYCIOĞLU’S REVIEW OF TIMUR KURAN, ED.,
*SOCIAL AND ECONOMIC LIFE IN SEVENTEENTH-CENTURY ISTANBUL:
 GLIMPSES FROM COURT RECORDS*, VOLS. 1–10 (*IJMES* 47 [2015]: 625–27)

TIMUR KURAN

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The August 2015 issue of *IJMES* reviews a ten-volume compilation of 17th-century Istanbul court records, issued between 2010 and 2013 under my editorship. The review offers useful quibbles that highlight differences between the priorities of historians and those of analytical social scientists. These comments fall within the boundaries of healthy academic discourse.

The review also contains claims that grossly misrepresent the entire enterprise, calling into question the compilation’s usability in statistical research. In point of fact, the

compilation provides seven comprehensive data sets according to parameters stated in the general introduction and seven thematic introductions. Every phase of the selection process abided by scientific principles, with the express goal of providing comprehensive data sets suitable to rigorous research, including statistical analysis. Yet somehow the reviewer seems to have misunderstood the scope and principles of the enterprise. He states that my introduction to the compilation provides “no systematic or scholarly explanation and statistical methodology” for “what the parameters of inclusion/exclusion of documents are, beyond the personal conviction of the editor about which documents interest economic and political historians.”

Let us unpack these assertions. The compilation’s general introduction says: “Cases are presented chronologically by topic. For each topic, every single relevant case is included, except for the very few whose record is incomplete” (vol. 1, p. 49 in English; vol. 1, p. 19 in Turkish). As also explained through concrete examples, a case recorded within one of the fifteen registers was included in a particular thematic data set if it falls into a topic, and omitted otherwise (vol. 1, p. 51–52 in English; vol. 1, pp. 22–23 in Turkish). A case that falls into more than one topic would make multiple appearances in the compilation, once in each separate database. Thus, a dispute involving a commercial partnership and a guild would go into both “commercial partnerships” and “guilds and guildsmen.” The handful of omitted cases—out of many thousands—were unusable because of moisture, worms, or tearing (vol. 1, p. 52 in English; vol. 1, p. 23 in Turkish). The rationale for excluding damaged records was precisely that a substantially incomplete case is worthless statistically. If we erred, it was on the side of inclusion. If only a word or two was illegible or missing, the case was included with appropriate markings (vol. 1, pp. 15–16 in English; vol. 1, pp. 13–14 in Turkish). Nothing was excluded due to “personal conviction.”

As the review notes correctly, the volumes are divided by topic, with some topics spanning multiple volumes. Thus, in our fifteen registers every one of the 411 cases involving one or more commercial partnerships appear together in one volume to form a data set usable independently of the other six. Moreover, since the cases are transliterated and summarized, a researcher may work with a full subsample covering, say, only the cases limited to Muslims, or only those that resulted in a trial. The compilation records 100 percent of the usable cases also for each of the other six topics: guilds and guildsmen, communal affairs of Christians and Jews, state–individual relations, waqfs, and credit relations involving the explicit use of interest through a legal euphemism.

Where my discretion was involved was in the choice of topics to cover comprehensively. I chose to produce data sets in areas where the need is acute for statistically rigorous research in Ottoman and Islamic economic and political history. The preface makes clear that the compilation is among the side products of a broad research project. It indicates that the registers were transliterated to serve a substantive research project, not to generate data sets for their own sake (vol. 1, pp. 7–10 in English; vol. 1, pp. 1–4 in Turkish). We decided to share with the research community data that are generating numerous new insights, as revealed by many recent studies, some published or under review, and others still ongoing. The systematic empirical research in these works has required digital coding of details available in the transliterated court records, which are presented in full within each data set. Provided

he or she obtains the records for the few unreproduced Arabic transcripts from the early part of the century, any scholar may perform the same statistical coding using the printed volumes at issue here rather than waiting for the release of their digital format.

The composition of each topic is explained in its thematic introduction. Five of the groupings involved no ambiguity because they relied on the presence of a legally defined player. Thus, a case is included in the “foreigners” data set if a foreigner is mentioned, and in the “waqf” data set if a waqf is mentioned. Of the two exceptions, one is the “communal affairs of Christians and Jews.” The grounds for inclusion are explained at length in the topic’s thematic introduction, which also contains a table listing the subtopics, making even clearer what we were after (vol. 1, pp. 437–40 in English; vol. 1, pp. 433–36 in Turkish). In brief, the goal was to exclude cases involving ordinary interpersonal transactions (as opposed to communal matters). One or two excluded cases might turn up to have had communal overtones on account of contextual information someone might find in a source other than the registers themselves. But using outside information about actors in the records would have biased the data set because contextual information depends on the analyst. It would have compromised the systematic nature of the data set. In other words, it would have led precisely to the arbitrariness that the reviewer erroneously detects in the volumes at issue.

The other topic possibly subject to ambiguity is the one that the review advances as evidence of arbitrary selection: state–individual relations. Our introduction to this topic explains that the data set includes

694 cases in which either a state official is a participant or taxation is at issue. As one might expect, the two types of cases overlap substantially, because state officials often appeared in court to register a tax agreement, or sue a subject over unpaid taxes, or defend themselves against an accusation of unjust taxation. (vol. 3, p. 15 in English; vol. 3, p. 3 in Turkish)

Where ambiguity could arise is in identifying who is a state official. We considered as a state official anyone with a title indicating that he works for the Sultan’s bureaucracy, military forces, or religious establishment. If an individual had some secret connection to the state—if he was in the Sultan’s pay as a spy, but appears as an ordinary subject in the legal records—this would have been missed. But that is not the reviewer’s point. He finds it “confusing” that under this topic “one encounters documents pertaining to taxation, inheritance, confiscations as well as trade.” Nothing here should confuse a reader familiar with Ottoman history. The Ottoman state interacted with its subjects in all these realms. In statistical work, there are ways to control for the different contexts. Besides, the diversity of topics is an advantage because it enables us to test whether state–individual relations differed across contexts.

Adding volumes on additional topics would indeed have been desirable, but it does not diminish the value of the seven comprehensive data sets offered in ten bilingual volumes running 6,431 pages. I take full responsibility for the “several cases” out of many thousands that contain an error, or arguably a misjudgment, in one part of either an English or a Turkish summary.

The selection of fifteen registers did pose a challenge. One might expect a sample distributed uniformly across the one-hundred-year span and Istanbul’s sixteen courts

to be the ideal. Unfortunately, quite apart from serious bureaucratic obstacles, several long chronological gaps in the surviving records make this impossible. In any case, to identify statistically whether systematic differences existed across courts, one must have a sufficient number of cases by court and by topic. This consideration led us to limit the data sets to just two courts. Since the substantive focus was commerce, we chose Galata and Central Istanbul, which carried a disproportionate share of the commercial workload in the capital (vol. 1, p. 49 in English; vol. 1, p. 19 in Turkish). Eight of the fifteen chosen court registers came from the twenty-three extant items of the Central Istanbul court. The remaining seven belong to the Galata court.

The reviewer worries that the selection of court registers lacks representativeness. While it is true that the courts of Galata and Central Istanbul are not representative of all of Istanbul, and certainly not of the Ottoman Empire as a whole, this is precisely the point. As the introduction makes clear, the courts were selected with the aim of studying the mechanics and dynamics of the Ottoman economy where structural transformations, if they occurred, would most likely be observed first (vol. 1, pp. 7–8 in English; vol. 1, pp. 1–2 in Turkish).

Court registers vary greatly in size. For periods where the registers are unusually thin, several consecutive registers were selected in order to maximize the chances of repeat appearances by individual actors. Given the various constraints, most notably that Central Istanbul's surviving registers form three chronological clusters, we ended up approximating uniform distributions for each court by choosing registers from the beginning, middle, and end of the 17th century for each court. In making the selections, we worked with the Islamic Studies Research Center to avoid registers already available in the public domain in transliterated form. In anticipation of statistical work, we took care that the Galata and Central Istanbul registers do not overlap temporally. Because of this feature, when our data are subjected to statistical regressions, the "fixed register effects" and "fixed period effects" are identical. Had we allowed temporal overlaps between the Galata and Central Istanbul registers, the two types of fixed effects may have differed; and, given the sizes of the data sets involved, we would have lacked the statistical power to distinguish between regularities associated with particular court registers (for instance, the personal biases of a certain judge who handled the cases in a given register) and regularities associated with periods (for instance, that one register dates from 1664 to 1665, which saw a major fire).

The decision to categorize the cases by function through a system different from the standard classification in Ottoman studies reflects considerations central to the discourses of social scientists. One is the distinction between state-initiated and subject-initiated actions; another is that between pre- and postcontract moves on the one hand, and moves during contract execution on the other hand. Fortunately, the customary classification system of Ottoman studies is obtainable at the click of a mouse once the published records are coded. Besides, the two classification systems are not rival ways of expanding knowledge; they are simply complementary ways of producing historical insights. Just as social classifications by religion, ethnicity, age, and education have complementary academic uses, so Ottoman and Islamic history benefits from broadening the disciplinary approaches through which data are collected, classified, and analyzed.